originality with which they are discussed, much valuable information on many intricate points which it would be difficult if not impossible to find elsewhere. The influence of Lake Ontario is seen in the diurnal changes of the wind, which in July is nearly S. from 10 A.M. to 3 P.M., W. at 5 P.M., nearly N. at midnight, about which it remains till 9 A.M, when it rapidly shifts to S.W., and ultimately to S. at 10 A.M. From October to March, when storms are most frequent, the greatest depression of the barometer and increase of vapour occur with winds from N.E. to S.S.E., and the greatest rise of the barometer and diminution of vapour with winds from W. to N.N.W. On the other hand, in summer the greatest depression of the barometer occurs with winds from E.N.E. to E.S.E., but the greatest increase of vapour with winds from E.S.E. to S.S.W. Most of the light falls of rain occur with winds from N.E. by S. to W., and of snow with winds from S.W. by N. to N.E.; most of the moderate falls of rain with winds from N.E. to S.S.W., and of snow with winds from N.N.W. to S.E.; and most of the heavy falls of rain with winds from N.E. to S.S.E., and of snow from N. to E.S.E. The importa nt bearing of these facts on the question of North American storms as well as on the climate of no inconsiderable portion of that continent is evident. Tables II. and XX. giving by interpolation-formulæ the mean temperatures and mean pressures of different days of the year, while of very slight scientific value, may be found to be useful in a meteorological office, but a simpler and in everyway more preferable table of normal daily values for pressure and temperature could be constructed from the arithmetic means of the thirty-one years' observations treated by Bloxam's method of averages.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

"Tone" and "Overtone"

IN the very favourable estimate of the work I have done in my translation of Helmholtz, in your number for Sept. 23, I am aken rather severely to task for my use of "Sensations of *Tone*" on my title-page, and my refusal to use the expression *overtones* in the body of the work. The title was long a matter of anxious consideration to me, and I have not yet seen my way to improving it. True, practical musicians, physiologists, and artists have each their own, very different, technical meanings for *tone*. The two last generally use it without an article, and in the singular; but musicians are accustomed to speak of "*a* tone," or of *several* tones, when they allude to musical intervals. In common speech, however, all three agree with the outside world in speaking of a "loud and soft, gentle and angry *tone* of voice," of a "fine-*toned* instrument," of the "splendid or miserable *tone* produced by a violinist," of the "magnificent *tones* of the organ." That is, we are all accustomed to use *tone*, as I have done on my title, for "a musical quality of sound." I know no other single word in English which expresses the same conception. In the original German, Prof. Helmholtz (and after him Prof. Tyndall) endeavours to use *tone* for a "simple tone" only. Neither have contrived to be consistent in so doing. I have had to correct the text several times in my translation on this very point, and instead of using *tone* for "simple tone" only, which is a new conception, and *clang* (in English, a *din*) for "compound musical tone," which is also a new and not an easy conception, I have invariably used the word *tone* (except when distinguished by a capital letter—thus, *Tone*, for the interval) in the usual general sense of the word, and distinguished the particular cases by the prefix "simple" or "compound." It seems to me that this is not so much "a little waywardness" on my part, as a desire for scientific accuracy.

As to "overtones," it is well known to those who, like my reviewer, are acquainted with the work in the original, that Helmholtz's expression "Obertöne" is a mere contraction for "Obertheiltöne" or "Oberparzialtöne," both of which terms he not unfrequently uses, and these are literally rendered by my "upper partial tones." Waiving my strong linguistic objection to the term "overtones" as an English word, my scientific justification for not using it in my translation must be sought for in the fact that even the German "Obertöne" has led Prof. Helmholtz himself not unfrequently to its inaccurate use for "partial tones" simply, including the lowest partial tone, which the word was especially invented to exclude. Singularly enough, even my reviewer has many times fallen into the same error (NATURE, p. 451, col. 2) in speaking of the "overtones" of a pianoforte string. Thus he says, "the first six overtones are all audible," which is not correct; but he means "the *lowest* partial tone and first *five* of the *upper* partial tones," or briefly "the first six partial tones," which *is* correct. Again, he says, "the seventh and ninth (overtone) which are *inharmonious*, &c., which is not correct, for the *seventh* and *ninth* overtones are the *eighth* and *tenth* partial tones, and are perfectly harmonious; but he meant the seventh and ninth partial tones. Again, he cites from p. 126 of my translation, the relative force of the first six "partial tones," as they are there called, but refers the table to the first six "*over*tones," which is altogether incorrect. Now if such men as Helmholtz, who invented the term, and as my reviewer, who uses it familiarly, can be led by it into what with them are mere inaccuracies of *expression*, must we not look to the utmost confusion of *thought* among persons to whom the whole subject is new, and who employ the term with a very vague or loose con ception of its meaning? In point of fact, many such cases have come to my notice. Hence, again, I cannot agree to think that my deliberate rejection of the word "overtones" is "the chief fault" or "a blot on the translation," but rather submit that it is a consistent endeavour to attain scientific accuracy of expression, and avoid confusion of thought.

I thank the reviewer for his generally favourable estimate, gladly accepting his rectification of the accidental Germanism "the musically beautiful" for "the beautiful in music," and I apologise for the length of this communication on the ground that it is not a merely personal vindication,

Sept. 25 ALEXANDER J. ELLIS

Colours of Heated Metals

I HAVE just watched the casting in gun-metal, in an engineering establishment in this town, of what is intended to be the rudder-post of a large vessel, which when completed will weigh about three tons. As the casting was a simple one, it was accomplished very quickly, and as the contents of the huge four-ton ladle were emptied into the mould, the dazzling stream of the metal flowed in a large volume over its lip. Brilliantly glossy it appeared as it broke through the folds of thin dross with which its surface was encrusted; and this it did at the lip of the vessel, while fold after fold of the encrusting pellicle was swept down the stream, and left behind it a straight or ragged edge of the thin film, from underneath which the metal welled out for a moment with an appearance on the surface of perfectly transparent purity. The appearance was a deception arising from the strong bluish-green colour of the light emitted by the pure surface of the metal, which I have never seen exhibited under similar circumstances by melted iron or steel. It extended also for only a short distance from the encrusting edge, the green colour soon passing into white, or paler green, where exposure to the air enveloped the metal again in a rapidly increasing film of oxides that tarnish its surface and render the stream white, or nearly so, in every part, excepting in a bluish-green ring, or border where the fresh metal made its appearance, and flowed over in a beautifully coloured stream from the mouth of the ladle. The strongest patches of the colour there were transient, the film of oxide apparently soon thickening enough to eclipse it, and by connecting itself to the broken edge of the thin film in the pot to tear away another fold, when the characteristic greenish glow of the metal immediately presented itself along the freshly-broken edge. I had watched and thus interpreted this beautifully varied play of natural colours in the molten stream for some time before it occurred to me that the peculiar hue of the freshly-exposed surface of the metal, glowing as it does with the brightness of what in the black film of oxide appears as white heat, is no other than the very colour of the heated metal which the theory of exchanges would lead us to expect. For as the colour of gun-metal in a cold state is yellow, the selective absorption of its surface in that condition must be exercised chiefly upon rays occupying the blue portion of the spectrum, and consequently in the heated state these rays

are emitted in excess; or if the heat is sufficiently intense to produce them largely, as in the melted metal, where the thin films of oxide on its surface glow with perfect whiteness, the metal itself must shine with bluish, or it may be with greenish-blue light, if the heat is only high enough to make the excess of green rays very strongly visible. If this should be, as I suppose, the real explanation of the very curious appearance of depth of a certain tint of colour, contrasting strongly in some parts of the melted stream by its greenish hue with the surrounding redder lights, according as the natural tinted appearance of the vivid metal is effaced or diluted by the floating films of white-hot oxides in lines and parts of the stream depending on the surface-flow, and suggesting in some degree the idea of a transparent cascade, and even from its colour of a waterfall, the process often repeated in large foundries of running gun-metal into large castings presents an instance of well-defined action of the law of exchanges which must be constantly witnessed and noted inquiringly by daily observers, and which certainly presents, if a different and more natural explanation can be given of its origin, to eyes unaccustomed and unprepared to receive it, a somewhat surprising and otherwise unaccountable appearance. In gun-metal, when the proportion of zinc introduced is very small, the coating of the melted surface by copper oxide is comparatively slow, and in melted brass it might not be possible, from the rapid oxidation of zinc upon the surface, successfully to observe the same phenomenon. In order to render melted copper fluid enough for casting, a small proportion of alloy sufficient to give it almost the colour of brass is required to be mixed with it, and large pourings of the pure metal cannot commonly be made; but perhaps in small castings of this metal, and probably also in those of gold, opportunities would present themselves similar to that which I have here attempted to describe, of verifying the same general law of radiation connecting together the qualities of luminosity and absorption in the surfaces of highly coloured metals.

Newcastle-on-Tyne, Sept. 20 A. S. HERSCHEL

Changes of Level in the Island of Savaii

WHILE feeling some diffidence about setting myself in opposition to so careful an observer as the Rev. S. J. Whitmee (NATURE vol. xii., p. 291), I cannot allow his statements in regard to changes of level in the island of Savaii, Samoan group, to pass altogether unchallenged. In the month of June 1874 I spent some weeks on the island, during which time I travelled around nearly the whole of it on foot. Though not a scientific observer, I was on the look-out for indications of change of level along the coast, and it is my decided opinion that such indications are quite as little apparent in Savaii as in Upolu. Mr. Whitmee, whom I had the pleasure of meeting on the island, directed my attention to what he believed to be a line of upheaved cliffs a couple of hundred yards back from the sea, near Tufu, on the south side of the island. On examining the place, after parting from Mr. Whitmee, I particularly observed that the floor of volcanic rock at the base of the cliffs bore exactly the appearance of lava that had cooled in the open air. The creases and ripples left on the surface of the lava in cooling were distinctly visible, which could not have been the case if the rock had ever been exposed to the action of the waves. No doubt was left on my mind that the floor of volcanic rock between the base of the cliffs and the sea was at one time on a level with the top of the cliffs, and that it had broken away and sunk several feet, from some cause which I do not attempt to explain.

I brought away the impression that Savaii was at one time much more fully supplied with barrier reefs than at present, and that recent lava-flows had extended the island out beyond the reef. So far as my observations extended, where reefs do exist they are terminated by points or capes of volcanic rock, looking as if the lava had overflowed and cut off the reef.

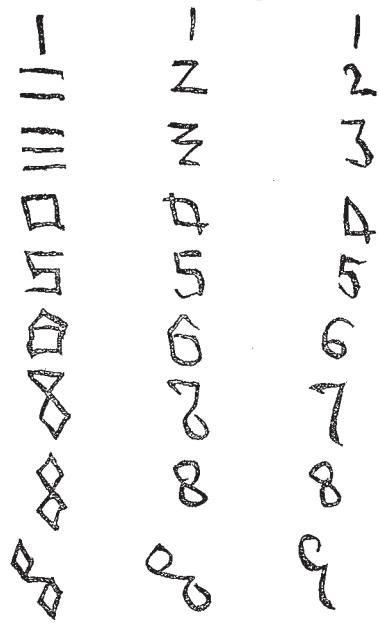
One circumstance almost, if not quite, fatal to the theory that Savaii has been upheaved in whole or in part in recent times, is that nowhere are there any signs of coral *in situ* above the sea-level. In this respect it is very different from the island of Rarotonga, in the Hervey group, which has most unquestionably been upheaved several feet, at least on the south side. There the barrier reef is altogether out of water, and what was once the enclosed lagoon is in some places dry land.

In regard to the absence of barrier reefs in front of lava-flows, I venture to suggest that it is more likely to be caused by the depth of the water or by the recency of the lava-flow than by any effect of existing submarine volcanic action on the coral insect.

San Francisco, Sept. 7 RICHARD WEBB

Origin of the Numerals

HAVING never met with any explanation of the origin of the numerals, or rather of the figures symbolising them, perhaps I am right in supposing that nothing satisfactory is known of it. In that case the following may be interesting to your readers. The first column contains the original figures, each containing as



many lines as the number which it is intended to represent. The other columns show the transitions likely to result from quick writing. W. DONISTHORPE

17, Porchester Terrace, W.

Pugnacity of Rabbits and Hares

I HAVE occasion just now to keep over thirty Himalayan rabbits in an outhouse. A short time ago it was observed that some of these rabbits had been attacked and slightly bitten by rats. Next day the person who feeds the rabbits observed, upon entering the outhouse, that nearly all the inmates were congregated in one corner, and upon going to ascertain the cause, found one rat dead and another so much injured that it could scarcely run. Both rats were of an unusually large size, and their bodies were much mangled by the rabbits' teeth.

I never before knew that domestic rabbits would fight with any carnivorous antagonist. That wild rabbits never do so I infer from having several times seen ferrets turn out, from the most crowded burrow in a warren, young stoats and weasels not more than four inches long.

It is evident that the show-fight instinct cannot have been developed in Himalayan rabbits by means of natural selection, but it is no less evident that if it ever arose in wild rabbits it would be preserved and intensified by such means. And in this connection I should like to ask any of your readers who may be able to supply information upon the point, whether there is any difference between the hares of Great Britain and those of the Continent with regard to pugnacity. I have been assured by Germans that in their country a hare will fight a good-sized dog rather than run, and that it is dangerous to handle a wounded individual. I do not know, however, whether or not to trust these statements, and as there appear to be very few examples of local varieties of instincts, it is desirable that anyone who can should either confirm or deny this curious instance.

Dunskaith, Ross-shire GEORGE J. ROMANES

OUR ASTRONOMICAL COLUMN

"35 CAMELOPARDI," B.A.C. 1924.—The principal component of this double star is not included either amongst the certain or suspected variables in Professor Schönfeld's last catalogue, but there would appear to be sufficient evidence of change to justify its being placed in the former class. Variability was suspected by the Baron Dembowski from his own estimates of magnitude 1865-